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Interactive comment on "Improved method for linear carbon monoxide simulation and source attribution in atmospheric chemistry models illustrated using GEOS-Chem v9" *by* Jenny A. Fisher et al.

Anonymous Referee #2

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The manuscript 'Improved method for linear carbon monoxide simulation and source attribution in atmospheric chemistry models illustrated using GEOS-Chem v9' by Fisher et al. describes an update of the linear CO method which is available in the GEOS-Chem model. Details about the technical implementation (usage of CO production rates from a prior model run instead of yield rates for NMVOC emissions), as well as an comparison of the 'old' and 'new' version with observation data are given. In addition an example of the new source attribution capabilities of the method is presented. As the paper describes an improvement of a current model it is in general

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Discussion paper

well suited for GMD, before publication, however, major revisions are necessary.

General comments:

The manuscript is in general well written, but especially the description of the model details lacks in clarity. I would suggest to condense the evaluation (more details below) and to describe the model updates as well as the general performance of the model with respect to changes of the CO production rates in more detail.

The authors seem to describe three main updates. The first updates are the improvements described at the end of the Sect. 2.2. These changes are applied in both presented model simulations. Accordingly, the term 'original CO-only simulation' is somewhat misleading as these model results are different from the 'CO-only' method which is the 'status quo' prior to the changes described by the manuscript. The main update are the improvements described in Sect. 2.2.2. The description of these updates seems insufficient to me. For readers which want to implement this method in their own model implementation details are missing. For people of the GEOS-Chem community which might be interested in using the method a user manual or similar (in the supplement) is missing. In addition not all simplifications/assumptions are discussed in detail (see below for details).

In general I would recommend to give proper version numbers to the different updates to make the model changes clearly traceable (e.g. 1.0 original Version, 1.1 updates described in Sect 2.2 etc.). To track the model development it would also be very interesting to show the difference of the model results between 'version 1.0' and 'version 1.1'.

Further I am missing a discussion about how the inter-annual variability of the production and loss rates saved from the full chemistry model influence the results of the 'improved CO only' simulation. Especially as the author state that results from one simulation year are sufficient for the improved model. For interested users it would be very important to see, how critical these pre-calculated production and loss rates are. If dynamics and pre-calculated production rates stem from different years they might not fit to each other and errors/biases are introduced. Please discuss these simplifications. Especially when thinking about sensitivity studies with changed biogenic emissions (e.g. to check the influence of additional source) a new calculation of the secondary CO production (with the full model) seems crucial to me. In this context it would also be interesting to show how large the changes with the new OH fields instead of the OH field used in previous version of the CO-only simulation are.

One problematic thing about the described update is that the amount of needed simulations are doubled. What is the benefit of using first the full model and then the CO-only model? In this context a discussion about the influence of the used CO production rates is important to see if results for different years are dominated by meteorological variability or by variability of the CO production (which of course is also influenced by the meteorological conditions). Especially if users are interest in one specific year it would be interesting to add the source attribution capabilities to the full chemistry model. The source attribution can then be calculated directly during the full model run and the second run (CO only) would not be necessary.

The comparison of the 'original CO' and 'improved CO' to the results of GEOS-Chem using the full chemistry clearly show the improvements of the new version. The authors might discuss shortly that the good agreement between the full chemistry and the 'improved CO only' runs can be expected, as same dynamics, chemical tendencies, emissions and OH-field from the full model run are used by the 'improved

CO only'. Given the small difference between all three model simulations compared to the observations presented in this Section it can be condensed a lot. Of course, a comparison to observation is important. As the differences between all versions are much smaller as the difference between observations and model most of the figures might be moved to the Supplement and the text of this Section can be condensed to the most important findings.

Specific comments:

Sect. 2.1: Please provide a table with the different performed simulations. Instead of 'improved CO only' etc. I would recommend more handy names of the simulations. In general this Section might be moved to the end of Section 2, meaning that first model improvements and at the end the model set-up are discussed.

Sect. 2.2.2: Please clarify which production rates are written out from the full chemistry model run. From my understanding only the total CO production and the methane loss rates are written out, from which the production of CO from NMVOC are then calculated off-line. Meaning you solve the eqn:

$$\frac{dCO}{dt} = E + PCO_{CH4} + PCO_{NMHC} - k[OH][CO] \quad (1)$$

If so, please clearly state the equation you are solving in the description of the improved model.

Figure 1: The figure could be improved a lot by using different color scales for the surface and 500 hPa. Please indicate also the differences in percent in the text.

Section 4: Why are you only evaluating values for the year 2009, while the years 2010 and 2011 are also simulated. Is the difference to the observations in these years similar or different from the values presented for 2009?

The used color coding of Fig. 4, 5 and 6 is problematic. Especially the difference between the blue and the purple color is very hard to see.

P10L1ff: In my opinion it is very problematic to refer to additional graphs (and software) which are available somewhere on the internet. In some years these URLs might not be valid any more. If the graph/software are relevant for the publication then they should be part of the electronic supplement (or get a DOI in any other way).

Figure 4: Please indicate the years for which the observations are averaged. Again, I don't understand why the observations are averaged over many years but the model results not.

Figure 6: The text and symbols are rather small. Are the model results sampled at the same time as the observations or are 'simply' daily average values of the model used when observations are available.

Figure 7: Please clarify - Anthropogenic and biomass burning does only consider primary emissions of CO, right? All production of CO by NMVOC (anthropogenic, biomass burning and biogen) are part of the NMVOC oxidation. If so, please clarify the sentence line P16L9ff.

P19L16ff: Please clarify this paragraph. Do you mean that the improved model should

perform better as the full chemistry model or are you discussing the small difference between the original and the improved model?

Technical comments:

P5L6: Please give a proper reference to the used JPL version.
Figure 4 Title: This figure does not show vertical profiles.

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